Marseille, 3th April 2023

Ref. egusphere-2022-145: Viticulture extension in response to global climate change drivers – lessons from the past and future projections

Dear editor,
Please find below our point-by-point replies to both reviewers. The comments are in red and the replies are in black.
Best regards

For the authors,

Joel Guiot

Replies to comments of Reviewer #1

I also found the study technically rather convoluted, and I am left wondering if, at least the part of the study referred to future conditions, could not have been more easily conducted by just directly analysing the output of global (and regional) climate simulations and applying, for instance, the VI index to those data. I guess the reader would scratch their head wondering why an emulator and an indirect estimation of the impact of external drivers is needed in the first place.

It is true that, if the only objective was the future viticulture, it should be more straightforward to calculate VI on the model simulations, and that the emulator was not necessary. But we want also to produce such analysis for any period of the past and to compare the future to the past. The emulator was build for that. Viticulture is one example of application – and it is important for stakeholders – and we have tried to show that it is easy to extend the analysis to other examples. Another objective was also to show that our approach is also a good attribution method. We have tried to make that clearer in the abstract and in the last paragraph of the introduction.

1) I think that the justification of the methodology will not be clear for many readers. The method is rather convoluted, involving an ensemble of simulations with Earth System Models, a statistical downscaling model, a combination of model output and proxy-based reconstructions using a Bayesian framework. Is this complex methodology necessary at all ? why not just take the output (e.g. median) of the ensemble of climate models ? Is the data assimilation needed to somehow correct the possible errors of climate models ?

Concerning the necessity to make these complicated developments, we may make two remarks: (1) the ESM provide very useful climate projections related on various forcings, but we have no guarantee that they represent the true world; the paleoclimate data enable to constrain the ESM to converge towards to this real world; (2) the ESM are very time resource consuming. So we have to adopt this strategy of fast running emulator constrained by data to achieve our objectives. We tried to improve our explanations in the first two paragraphs of section 2.

2) Some methodological steps are too shortly explained. The climate reconstructions used in the data assimilation step appear almost out of the blue. They should be included in the

material and methods section. Perhaps also the data assimilation methodology should be explained here as well.

We have added a paragraph at the beginning of section 2.8 (paleodata assimilation). As well the climate reconstructions than the methodology are presented in the material and methods sections, i.e. in Table 7 and section 2.8.

3) Section 3.3 (independent validation) raises some questions that the text simply glossses over. The text asserts that the agreement between the independently reconstructed PSDI and the model output is good, but Figure 9 does not convey this impression. The assessment is essentially visual, comparing the panels in Figure 9, but the colour coding used does not really help the reader to see the similarties and differences. Admittedly , colour bars are rather subjective, and in my experience some readers find some useful when other readers find them difficult. Here, however, it is difficult to distinguish the colour tones. E/EP for 1300, for examples, is just blue everywhere. Would it be possible to use, say , 10 hues that the eye can easily separate ? To me, the panels for 1300 BP look very different, also the agreement shown in the panels for 1000 PB and 700 BP is questionable. I believe the authors that the data sets may agree, but the pictorial representation is really not adequate to convince the reader.

Thank you for this remark which is very important. As we told in the ms, it is difficult to calculate quantitative statistics. We have modified the color codes with tones from the (red, yellow, white, green, blue) palette. We think that the maps are clearer and the coherency between them appear better (even if it is not perfect).

6) 'Figure 4: Note that this value underestimates the true earth surface temperature because our mean is based on the equirectangular projection which gives too much weight to the high latitudes.'

What we have done is to calculate a raw mean based on the gridpoints available. We agree that is should be more rigorous to calculate a weighted mean, taking into account the area of the gridpoint. This figure is just indicative and has no effect on the final results, especially because we will work in the next steps on a region where all the pixels have almost the same area. We have decided to modify the figure and to present the anomalies instead of the raw values. This does not remove the biases but it avoids to focus on a secondary problem.

7) 'The sunshine percentages are obtained by linear regression on temperature and precipitation (Guiot et al., 2000)' Models produce downwelling solar radiation at the surface. So why use an indirect approach ?

It is true that the surface solar radiation available from the ESM is a much better variable than the % of sunshine for each month requested by BIOME4. It is the way of working of BIOME4 which has been validated with this approximation. Moreover, we wanted to limit at maximum the number of variables to be estimated by the emulator. So we have emulated temperature and precipitation and deduced sunshine from them, as we have done before in Guiot et al 2000 and following papers. In any cases, sunshine has a limited effect on the vegetation simulated by the Biome model and this point is not crucial.

8) …. Were these time series normalized prior to PCA?

Yes they are. Information added (l.331)

9) This process is known as a data-model assimilation (Goosse et al 2012) The data are the paleoclimate reconstructions and the model the emulator. Its statistical simplification makes it possible to run it thousands of times in a relatively short computational time as required by the assimilation methods (Widmann et al., 2010). We use a Bayesian approach called the Markov Chain Monte Carlo method (MCMC), which makes it possible to converge towards the best parameters in the sense of probability distribution (Hargreaves and Annan, 2002). I think this paragraph is written a bit sloppily, and it will also be unclear for readers not well versed with Bayesian methods. My interpretation is that the model and reconstructions are combined by using the Bayes theorem This application requires the computation of an integral and for this computation the authors used a MCMC methodology. More importantly, the need for this data assimilation step remains obscure. Why is it needed at all ?

We agree that this paragraph must be written more clearly. The objective of the data assimilation is to make converging the simulations towards the paleo data by adjusting the effect of volcanic and solar activities and so understand what forcing is the most important to explain the climate variations of the past periods studied. It is then intended to improve understanding of the forcing effects and not only to improve the predictions. We have added a paragraph at the beginning of section 2.8.

10) 'anomalies from the pre-industrial period for the 10 spatial boxes and the 9 time slices, obtained from pollen (Guiot and Kaniewski, 2015) and corrected/precised as indicated in Table 1 of the main text.'

Sorry it is a mistake, correct table is Table 3

11) 'the sum of the product of monthly temperature and precipitation for the growing season (Hyl),' The sum or the product ?

To make things clearer: the sum on the growing season of the monthly temperature multiplied by monthly precipitation (corrected at lines 35-36 of page 20)

12) (HI>1400, DI>-100, Hyl<5100 and Tmin >-17°C) : units are missing

This have been corrected

13) Some of the climate variables needed for these indices were not available from the BIOME4 outputs. However, other variables, such as those associated with the net primary production of plant types are very interesting because they include the CO2 effect on photosynthesis. Do they also include the effect of CO2 on water efficiency ?

Yes it is true; it will be precised (l. 442)

14) Equation of VI… The VI is validated by just comparing with the present climatology. It is a new index and apparently, there is no other type of validation. How can we be sure that this index can describe changes in potential viticulture well ?

This index is closely based on previous indices taken from the literature and is validated visually by comparing to the present viticulture extension. We indicate that it is satisfying except on the continental climates of Central Europe. Additional discussion is provided in response to comments of Rev#2 (precisions at beginning of section 3.4, l.552). Another is also that we focus on the changes in viticulture extension, and then it is more appropriate to compare the maps together instead of analyzing single snapshots.

15) ' Fig.6 presents the overall correlations between the emulator outputs and the proxy-based reconstructions.' The x-data and the y-data are in my understanding not totally independent. The emulator has used the reconstructions in the data assimilation step, so it is not totally surprising that they are correlated . Also the caption is not clear, specially this sentence: Temperature dots correspond to the 10 boxes of the 11 periods between 2500 to present and precipitation data to the two oldest periods (4200 and 3200 yr BP) and the present.' Does it mean ' Temperature dots represent the mean temperature in the 10 boxes in the 11 periods between 2500 BP until present and precipitation dots represent the mean precipitation in the two oldest periods (4200 and 3200 yr BP) and in the present,

Thank you for helping to improve the caption. It is true x and y should be ideally correlated with a R=1 (blue line), but as in standard regression approaches, this is only reached if the data assimilation is perfect. Ideally all the dots should be distributed along the blue line. The black line reveals that low temperatures and low precipitations are overestimated and that the high precipitations are underestimated (not the high precipitations). As this verification is not independent, an independent verification is done in Fig.9.

Replies to comments of Reviewer #2

General comments

The manuscript provides a valuable complementary approach to other climate change impact studies on grapevine extension areas under different climatic conditions and contributing to their robustness. The study is well supported with figures and tables on various modelling setups and outputs. Some parts of the paper need improvements, especially the descriptions of the applied methods, processing steps and limitations of the approach/results (see details below).

We are grateful for this positive general evaluation.

Specific comments:

-At the begin of method you may define "emulator" and "Bayesian framework" and how these are applied in your study. Fig.1. needs an overhaul, e.g. the meaning of the shapes are not explained, and it should contain more details on processing steps in a straightforward way. Include also the validation steps with tree rings. The accompanying description should be improved as well and make details more clear. Rewrite e.g. the "Calibration of the emulator", in context to Fig.1. You should also include the inputs and outputs into that scheme to make the process more clear.

The terms mentioned by the Reviewer are important and deserve to be defined in the head of the method. We have completed the caption of Fig. 1 with a definition of the hashes. We have added the validation step in the figure. We have tried to improve the accompanying description of the method at different places of section 2 (see replies to comments of rev#1).

-p5: Orbital parameters…this abstract needs better description/sentences.

We will add a few words on the physical meaning of these parameters (first paragraph of section 2.1)

-p11: Please outline in the description of BIOME its limitations e.g. how far weather/climate extremes are considered for impact on vegetation/grapevine and what are the relevant uncertainties? E.g. the MTCO for predicting frost resistance has quite high uncertainty when not calibrated for regional climates e.g. continental vs. Mediterranean, which is also visible in your results, where there is obviously a strong bias for continental climates (see below). Grapevine cultivars have a wide range of winter frost resistance: some cultivars can survive -30°C during winter dormancy, frost resistance is influenced also by fertilization and other grapevine management options (also relevant for the VI index description at p19). These limitations should especially also better be reflected in the results desriptions/limitiation and in the discussion. A further limitation of BIOME and your study is that it does not consider climate related biotic damage risks (you only mention it later as a limitation in your study).

BIOME4 simulates a mean vegetation state based on an average climate. The extremes are not taken into account. It is certainly a limitation which will be more clearly discussed. The example of frost resistance is another good example of limitation of the approach. Indeed, the difficulty to simulate continental grapevine has several explanations. As proposed, the absence of distinction between varieties is certainly one explanation (we have no idea of the cultivars used by the Roman farmers). The fact that VI is calculated using a mean climate is another one. Our approach is comparable to the approaches of the literature based on descriptive indicators (e.g. Malheiro et al, 2010) and cannot be compared to mechanistic approaches using phenological models such Sgubin et al (2022). We have added a paragraph at the end of section 2.9 to stress these limitations.

-Fig 10a vs. 10b shows that there is a strong bias in the continental areas according to predicted wine growth areas. Under the future scenarios this bias occurs compared to other climate change impact studies for wine production areas. As described in p25 that's based on the overestimation of low temperature limit (VI Index, MTCO), which was not calibrated for the continental region. Therefore these areas should be marked in the graphs better with an additional pattern maybe, and elaborated better in the description and discussion too.

Reviewer is right, our explanation based on microclimates is not fully adequate and we will clearly state that the VI does not work with continental climate because the condition on MTCO is too strong. We have added a sentence in section 3.4 (first paragraph) to clearly set this limitation: More generally, these discrepancies show that our index is not calibrated for continental regions with winters colder than 3°C (in average). We have also added a sentence at the end of item 4) of the discussion.